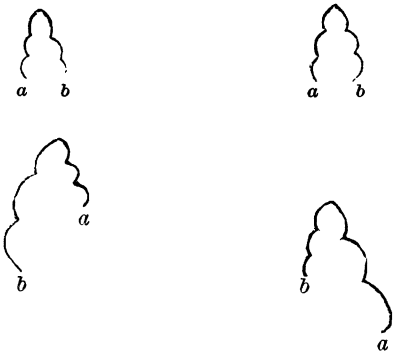
alone imagined to be in a state of perspiration. Any strong intellectual exertion produced a marked effect, even when the needle no longer responded to excitations of organs of sense; but a simple application of the multiplication table was not very effective. The effect of any contraction of a muscle was very marked. DuBois-Reymond first showed that a current is produced in the hand when the hand contracts, but he attributed it to the negative variation in the current in the muscle. It seems now that Herrmann is right in considering it to be a secretory current produced by the increased activity of the sweat-glands, for it is produced in every part of the body where there are glands, no matter where the muscular contraction takes place, and it cannot be detected in places where the sweat-glands are few or wanting. It is not so much the extent of the voluntary motion which regulates the amount of the current as it is the degree of conscious exertion that accompanies it; to fixate the point of the nose, makes a change of twenty divisions on the scale. If both points of attachment of the electrodes are well supplied with sweat-glands, the current is frequently found to move first in one direction and then in the other; if one only is so, the current is negative at that end. The existence of this current indicates an increased activity of the sweat-glands; it is well known that their activity develops the so-called secretory current. It is therefore proved that the course of nearly every kind of nervous activity,—from the simplest impressions and sensations, to voluntary motions and the highest forms of mental exertion,—is accompanied by an increased activity in the glands of the skin. The plethysmographic investigations of Mosso, François Frank and others show that the blood-vessels of the extremities take part in all kinds of nervous activity. But they contract, and hence they are not the cause of the increased activity of the sweat-glands. That must be due to a special excitation of the nerve-centers which regulate them. Its purpose is plain; all nervous activity means an increased accumulation of the products of decomposition and a rise of temperature; the greater activity of the sweat-glands facilitates the elimination of the products of decomposition, and at the same time serves to bring down the temperature. C. L. F.

Untersuchungen über die Orientirung im Fühlraum der Hand und im Blickraum. J. LOEB. Pflüger's Archiv, XLVI, S. 1–46.

Loeb has made the important discovery that when we will to make equal excursions of the hand in the hand-space, the excursion actually executed is shorter than that willed, if the muscle concerned in making it is shorter than in a state of rest; and is longer, if the muscle is longer. The method of conducting the experiments was as follows. A person stands before a vertical table and makes with one hand, say the right, an excursion along a thread of definite length. At the same time he wills to move the left hand (which is not guided by a thread) over an exactly equal length: if this hand starts from a point higher up than the other and moves *upward*, it moves *less* than it should do; if it moves *downward*, it moves *more* than it should do. The more the starting-point of this hand is depressed (the hand which follows the string remaining at a medium height), the greater are its upward motions, the smaller are its downward motions; but the lower the starting-point, the longer are the muscles which raise the arm, the shorter are those which depress the arm. A simple experi-

ment, by which anyone can convince himself of the sense of the illusion, is this. If, with the eyes shut, one attempts to draw a leaf with each hand, the two figures are very much alike, if the two hands are symmetrically situated. But if one hand is raised and the other lowered, the up-strokes of the one hand and the down-strokes of the other will be too short, or none of the four leaf-sides are drawn of the size intended to be drawn and supposed to be drawn.



The drawing goes each time from *a* to *b*.

The same thing holds for horizontal motions. But if horizontal motions are performed with the hand raised or lowered (but not horizontally displaced), the illusion does not occur; the muscles which effect *horizontal* motions are not put out of their resting-position by raising or lowering the hand. The error is very considerable in amount; the motion executed may be only one third as great or three times as great as that supposed to be executed. That it is the actual length of the muscle and not the tension it is under which causes the illusion, is proved by showing that the illusion is not produced by attaching weights to the hand; intended movements are executed very accurately even when the hand is restrained by a weight of nearly 6 kg.

Loeb finds, in opposition to Dr. Stanley Hall (*Mind*, No. XXXIII.) that gravity is of no effect in producing illusions of this kind; Dr. Hall happened not to try his experiments with the variable hand low down in its space, as well as high up, or he would have noticed that the illusion works against gravity as well as with gravity.

The muscles of the eye exhibit the same phenomenon; a distance looked over seems shorter in any direction, the further away it is in that direction. In indirect vision, the same thing holds, but with less exactness, and this leads Loeb to the conviction that the local-signs of the retina are simply inhibited impulses of the will. There seems to us to be a lapse of logic in this, as well as in all other statements of the same theory. Granted that we estimate the distance of an object seen indirectly by a consciousness of the strength of the innervation which would be necessary to make us look at it, how do we judge of the required strength of innervation? In other words, if the imagined impulse of the will is the cause of the feeling of distance, that must itself have a cause in some preceding sensation,

and nothing at all has been done to show that it is the impulse, and not *the sensation which determines the impulse*, that rises into consciousness, and effects our knowledge of the position of the object.

Loeb considers himself to have absolutely proved, by these experiments, that our knowledge of the extent and direction of our voluntary motions depends upon the impulse of the will to perform the motion, and not upon the sensations excited in the active organ of the motion. The two assumptions which the validity of this proof requires are, that no change in the sensations conveyed by skin, joint, tendon or muscle is produced by an unusual position of the organ (that is, that *sensation* is not a function of the present condition of the elements which communicate it, while the excitability of a muscle is a function of its state of excitation), and—what Loeb frequently affirms—that the constant hand and the variable hand are both moved by one and the same impulse of the will. To the consciousness of the present writer there seems to be, when the experiment is performed, the following processes going on in the mind. One is at once aware that, since the two hands are to start together and to end together, an accurate length is to be gone over by the variable hand by giving it the right velocity. During the tracing of the line (if it is done a little slowly) there is a constant estimation of the correctness of the velocity already executed (that is, a comparison of it with the velocity of the other hand) and a corresponding decision to increase the velocity or to diminish it for the rest of the excursion. When the hand is extended, the line is executed wrong; but how can one be sure that that is because a right degree of innervation has a diminished effect upon a muscle already contracted, and not because there is an error in the estimation of the amount of contraction which takes place in a contracted muscle? (It would be interesting to compare the steadiness of the velocity of the variable hand with that of the constant hand.) Nor does the assumption that both hands are moved by a single impulse of the will (and therefore by equal impulses) seem to us to be tenable. One is conscious, very plainly when the positions of the two hands are not symmetrical, of a rapid moving of the attention from one hand to the other, and that is, of course, the same thing as a rapid alteration of impulses of the will. Moreover, the two hands have totally different tasks to perform; the right has to follow a thread and to stop when it gets to its end; the left has to execute a perfect copy of the motion of the other. It is impossible that two such different commands as this should be executed by a single impulse of the will, nor is there any anterior reason for supposing that the two impulses required are necessarily equal in amount. The experimental results of Loeb's investigation are extremely interesting, but they do not seem to us to bear out his far-reaching conclusions.

C. L. F.

Ueber das mechanische Latenzstadium des Gesammtmuskels. DR. W. COWL. Separat-Abdruck aus den Verhandl. d. physiol. Gesells. Berlin. 1889.

In view of the difference of the results of Yeo's study of the "latent period" (see review, AMER. JOUR. PSY. II, 488) from those of Gad and more recent observers, the author has undertaken a repetition of Gad's experiments using the same methods and apparatus, and confirms his results, concluding that the "latent period" is due (though

perhaps not solely) to a stretching of the muscle preliminary to its contraction. He prefaces the statement of his own work by a brief historical summary.

Ueber den Muskelsinn. GOLDSCHEIDER. Verhandl. d. physiol. Gesells. Berlin. Sitzung am 17 Mai, 1889.

The hypothesis of a sensation of motion, distinct from that of the positions beginning and ending the movement, is supported by the following considerations. 1. The sensation of motion becomes clearer as the rapidity of the movement increases, and attends movements of too short duration for the complicated processes of a judgment from the positions. 2. The just observable sensation of motion accompanies movements so very small that their limiting positions are probably indistinguishable. 3. Sensations of motion are clearly perceived before the direction of movement is clear. 4. The sensation of position can be temporarily removed by faradizing without destroying the sensation of motion.

In experiments on the lifting of weights it is well to use only single segments of the limb. The physiological conditions of the experiment are thus greatly simplified, with the result that in lifting the weight by a thread nothing of the sensation of encountering at some moment the resistance of an exterior heavy object is felt (a prominent sensation in lifting with more than one segment,) but only the more subjective sensation of heaviness (*Schwere-Empfindung*), of greater difficulty in executing the previously easy movement. This sensation of heaviness has its seat in the tendons; that of resistance, like that of motion, in the joints, and is called forth by the pressure of the joint surfaces upon one another. It suffers if there is motion in the joint at the same time. In lifting weights in the ordinary way both sensations are aroused.

As against the participation of an innervation sense in these judgments the following facts are adduced. 1. The sensation of weight is felt when the contraction of the muscle is produced by electrical stimulation or reflexly like the knee-jerk. 2. The limb may seem perfectly relaxed when it is still partly supported by muscular tension. 3. Movements may be made actively as well as passively which are too small to be perceived by the subject, and the limit of perceptible movements is raised by faradization in one case as in the other. 4. Innervation sensations do not mediate the sense of position, for that is almost entirely destroyed by faradization. 5. Certain illusions exist which should not be possible with an innervation sense. The consciousness of voluntary movement comes from the immediate succession of peripheral sensations of motion upon the genesis of the corresponding motor image. (The weight of evidence at present is very strongly against the existence of innervation sensations, and those whose theory of space perception involves them will have to bethink themselves of reconstruction. REV.)

Zur Frage der psychophysischen Messungen bei Geisteskranken. M. K. WALITZKAJA. Archiv f. klin. u. gerichtl. Psychiatrie v. Merschejewski. I, 17. Rev. by Kraepelin in the Allg. Zeitsch. f. Psychiatrie. Bd. XLVI, H. 2–3, S. 245*.

These experiments were made on 7 insane and, for comparison, on 5 sane subjects. In 4 of the 7 cases the diagnosis was general paralysis, in 2 progressive paralysis, in 1 paranoia. Simple reaction-